

June 18, 1974

Professor H. L. Kornberg
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Dear Hans,

Your interest and concern about the problem raised in Maxine's and Dieter's letter to Science is most heartening particularly as there are many people here who also believe any expression of concern is likely to set genetic chemistry research back seriously and to infringe upon the traditional rights of free inquiry.

The outcome of the Gordon Conference letter (Maxine's) was to prod the Assembly of Life Sciences of the National Research Council into asking me to advise them on the issues raised by the letter, but to do it informally, i.e., "unofficial". I asked a group of people (David Baltimore, Jim Watson, Dan Nathans, Sherman Weissman, Norton Zinder and Richard Roblin were the ones who attended) to meet with me to discuss the matter and arrive at some recommendations for the Assembly. We met at MIT for a day and settled on the idea of calling a conference next February of those scientists working on methods of joining DNA molecules and particularly those involved in constructing hybrid DNAs. It was our plan that one of the major purposes of the Conference, besides a report on the scientific progress (the state of the "art"), would be a wide ranging discussion of potential hazards growing out of these types of experiments. Were there any experiments that should not be done? How could such a moratorium be proposed or enforced? In short, we expected a frank and searching review of what people were doing or wanted to do, particularly from the point of view of whether they should be done. But as we talked we realized that the pace of events might not wait for February and that some of the experiments many people would agree could be hazardous would be done by then (e.g., attempts to fuse portions of Herpes DNA to appropriate plasmids for cloning in *E. coli* were imminent). Since the technology for constructing hybrids has become ridiculously simple (see the April and May PNAS), that fear was well founded.

Consequently we decided to devise a letter to be submitted to Science and Nature calling on scientists to defer certain kinds of

Professor H. L. Kornberg
Page Two

experiments until these potential hazards could be better evaluated and certainly until there was an opportunity to discuss the issues at the February meeting. After the draft of the letter was constructed, revised and finally settled on the Academy suggested that it be submitted as a report of a Committee of the Assembly of Life Sciences. Enclosed is the final draft to be submitted to Science and Nature in a few days. It is for your information but I would ask you not to make it public or available except as a privileged communication. (The reason for this is some touchiness by the Academy, the journals who will publish it and one newspaper which is withholding publication of its contents until the journals appear.) If you would like to discuss its contents, implications or impact perhaps we can do it by telephone before you have to address the government committee. I'll be at Stanford the entire week of June 24th.

I've just returned from a trip in your footsteps! I was one of the 1974 Australian Biochemical Society Lecturers for the annual meeting in Adelaide. I heard much of your exploits-swimming in the shark-infested water around the Great Barrier Reef-wit, charm and beautiful science. It was a marvelous month and someday we shall have to compare experiences.

Is it permitted to offer my congratulations and best wishes at your having been offered and accepted the Chair in Biochemistry at Cambridge? Well, anyway, I do. And I hope on future visits to Cambridge to visit with you. I'm sure this portends a new era for Cambridge Biochemistry.

With best regards,
Sincerely,

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Enc.